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REVIEW AND APPRAISAL

Thomas A. Kochan
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MASSACHUSETTS
INSTITUTE OF TECHNOLOGY
50 MEMORIAL DRIVE
CAMBRIDGE, MASSACHUSETTS 02139

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Chapter 9

Appraising a Decade's Research: An Overview

Thomas A. Kochan, Daniel J.B. Mitchell, and Lee Dyer

If it is impossible to review a decade's research in a single volume then it is clearly also impossible to summarize all of the key developments in a single chapter. Therefore, in this final chapter we will only highlight a number of the key points raised by the authors in the previous chapters, compare the record established in the 1970s with the images we have of research from earlier time periods, and pose a set of challenges for industrial relations researchers to consider as they shape their research agendas in the decade ahead.

International and Comparative Industrial Relations

It is perhaps fitting that we start the volume with a chapter on international and comparative industrial relations research since this topic is potentially the broadest in scope of any of the subjects covered. It is also fitting since the cycle of activity and research output in this area appear to reflect what at least some observers (Strauss and Feuille, 1978) saw as representative of industrial relations research in general. That is, following the high level of activity described by Scoville in the early 1960s, the volume and visibility of international and comparative research declined through the mid to late 1970s. In recent years, there appears to be a budding revival at work. The relative importance of topic areas in the international field also changed and mirrors a message driven home in other chapters of this book. Broad issues concerning the theory of the labor movement and industrial conflict

were surpassed in importance by issues involving economic development and trade policies, workers' participation in managerial decision making, and the behavior and effects of multi-national organizations.

Still another area in comparative research that has been growing in recent years has been comparative wage determination. Econometric studies have compared international differences in the sensitivity of wage inflation to various explanatory factors. Institutional differences between the U.S.--with its widespread use of multi-year union contracts--and other countries (where multi-year contracts are rare) have received increased attention from labor economists.

It is likely that the current awareness of the effects of world-wide competition, the interdependence of national economies, and the popularized comparisons of differences in national systems of industrial relations and management process will further spur interest in comparative and international industrial relations research. Indeed, the recent flurry of popular books on Japan will likely give rise to additional and perhaps more detailed analysis of the diversity of industrial relations systems that exist around the world. The debate over whether industrial relations practices can be exported is clearly alive and well in the popular press. It may well re-emerge in the 1980s as an important topic of analytical research.

Wage Determination and Public Policy

Flanagan and Mitchell point out that the decade of the 1970s saw strong theoretical advances in neoclassical human capital theory and its application to various aspects of wage behavior through the use of larger and better data sets. While the evidence accumulated through this work allows the authors to reach a number of substantive

conclusions regarding various "stylized facts" about wage differentials, some things, such as the stability of wages over recent business cycles remain to be explained. Presumably, as the authors suggest, this might be done through a better integration of neoclassical and institutional or segmented labor market theory. Flanagan and Mitchell, like many others, see implicit contracts theory as a promising avenue of research for explaining some of these stylized facts and integrating neoclassical and institutional theories. One question that must be asked is whether implicit contracts theory is really a new idea or simply a new term for describing ever evolving employer personnel policies and collective bargaining practices. That is, employers and employees continuously or periodically adjust the mix of compensation and working conditions to meet their particular needs and maintain their employment relationship over time. Just as practices adjust with a lag to changing external pressures, managerial fads, and employee aspirations, the characteristics of the efficient but implicit contract that economists seek to model will also change over time. Economists will then be forced to live with the same amount of change as do other social scientists who study compensation practices.

Like other areas, wage determination researchers will, as Flanagan and Mitchell note, respond to shifting public policy interests and major economic events. Thus, we might expect to see research on public sector wage determination make a comeback in the mid 1980s as observers attempt to sort out the effects of the cutbacks in local and state government revenues on wages, employment, and the quality and quantity of public services. New estimates of the elasticity of public sector labor demand will be needed to

replace the estimates obtained during the period of relative stability and moderate employment growth of the late 1960s. In this area, as in private sector wage research, we are likely to see a new stream of studies that debate whether or not the recession and other economic and political events of the early 1980s produced a discontinuity in wage setting practices and outcomes in union and nonunion sectors. Here the analytic techniques Flanagan and Mitchell discussed as appropriate for estimating the effects of incomes policies on the wage structure and on wage changes will undoubtedly prove helpful.

Perhaps no area is more ripe for a new theoretical leap forward (or to put it another way, there is little perceptible net return to another study using the conventional methods and models of the 1970s) than the effects of unions on wages. The exhaustive reviews and critiques of this research published in recent years all show the need for a paradigm shift that is more firmly grounded in the dynamic adjustments unions and employers make over time under collective bargaining. Once the theoretical breakthrough is made, we can then again return to debates over the econometric specifications most appropriate for testing the theory. In the absence of a theoretical advance, even improvements in research design (such as more longitudinal analysis) are not likely to add much new insight to the existing body of evidence.

Employment and Training Policy

Barocci describes employment and training research in the 1970s as "an implosion", i.e., a turning in on the major theoretical and policy problems by concentrating on the methodological issues relevant to evaluating the costs and benefits of different government

programs. While progress has been made in clarifying what is not acceptable in evaluating these programs, consensus has yet to be achieved around either preferred methods of analysis or the effects of various programs. Thus, in a cost-benefit sense we are probably somewhat better off after spending millions of tax dollars for evaluation research in this area, but just how much better is hard to tell.

An unfortunate characteristic of the manpower research that developed out of the active manpower policy era of the 1960s and 70s was its failure to link evaluation research with the theoretical work on labor market behavior. For example, about the only place that theory intrudes on the evaluation studies is in the specification of control variables in earnings equations. Human capital models have dominated the choice of these variables. Yet we seldom ask what difference does it make to the design of employment and training policies whether one sees the behavior of workers and firms through the eyes of neoclassical labor market theory, segmented labor market theory, or some alternative perspective? What assumptions about the behavior of the firm and about human resource management policies and strategies are needed to guide an effective labor market policy? How does our employment and training policy fit into macro economic policies? How do we link the regulatory policies discussed in the Olivia Mitchell's chapter to these employment and training policies?

These issues will take on increased importance in the 1980s. Current political decisions to drastically cut back federal expenditures and to revamp the design of the employment and training system will force academics and would-be policy advisors to re-argue

the basic question of whether or not the U.S. economy requires an active employment and training policy. Answers to this question depend heavily on both one's theory of how labor markets work and empirical evidence supporting one's theory. Thus, the 1980s will likely be a time when we return to very basic theoretical and empirical issues in this area.

Given the ambivalent conclusions reached in the evaluation research of the 1970s, it is unlikely that we will want to return to the same programs. Nor are policy makers likely to be very tolerant of arguments for more experimentation in order to determine which program alternative offers the greatest net benefits. Indeed, given the "implosion" of the 1970s, it is not clear that researchers can realistically promise that answers would be forthcoming if such an "experimental society" were to be started up again in the 1980s. Future policy makers will be no more tolerant of arguments for "true experiments" rather than "quasi experiments" than their counterparts were in the past. But they will once again, as in the early 1960s, be searching for new ideas and strategies for lowering unemployment, improving the functioning of the labor market, and improving the employment and earnings experiences of the most disadvantaged individuals and groups. The question is: Will we as researchers be able to draw on current and/or previous research to provide theoretically and empirically grounded policy prescriptions? Now may be the time to begin the debate so that, for once, researchers lead politicians in the search for answers.

The Labor Market Effects of Federal Regulation

Research on the effects of federal regulations of workplace practices followed the theoretical and methodological pattern

established in other areas of labor economics research in the 1970s by adopting a neo-classical theoretical framework and relying heavily on econometric techniques. Olivia Mitchell reviews the results of a decade of this research. Policies arose out of a concern for workers affected by specific problems of discrimination, safety and health, pension insecurity, and followed a strategy of imposing specific restrictions on the behavior of employers. The evaluators of these policies, on the other hand, generally pressed beyond the specific problems to assess the affects of the policies on standard employment and earnings outcomes. The theoretical focus generally was one of asking how would these policies play themselves out in a competitive labor market, how do they actually influence employment and earnings outcomes, do they influence the specific labor market outcomes to which they were addressed, and then, on balance, are their specific accomplishments of sufficient social benefit to balance any social and economic costs which they may impose? Along side these standard neo-classical evaluations were a number of studies that looked at legal questions and the response to the new regulations by employers, unions, and other labor market institutions. Unfortunately, there were few studies that integrated both approaches.

By the end of the decade, therefore, we find that we have two parallel sets of literatures on most of these regulatory policies. One set is based on economic theory and tends to focus on the cost side of the regulations while the other is more legal and institutionally based and explores the benefits of these regulations, and their consequences for due process.

Some encouraging signs of a movement toward a better mixture of economic and institutional approaches is evident in those studies

that attempt to use micro (organizational and/or individual) data sets.

Personnel/Human Resource Management

Personnel research did not disappear during the 1970s as George Strauss predicted a decade ago. Instead, it re-emerged under a modified name, Personnel/Human Resource Management. Indeed, if the number of new scholars and job openings in major universities is any indication, this specialization is currently a growth sector within industrial relations. What accounts for the renewed interest? It probably has little to do with the theoretical or empirical advances in personnel research in the 1970s. Considerable progress was made during the decade in sharpening the conceptualization and measurement of standard personnel outcome variables and their correlates, as Dyer and Schwab point out. But personnel researchers tended to take a very micro (individual) and static approach to their topics despite the fact that its resurgence was stimulated primarily by changes in macro organizational and external forces. Among these were increased pressures for productivity improvements and reduced labor costs, more and more rigorously enforced government regulations, the tightness of labor markets for executive, technical, and some highly skilled blue collar workers, and the changing demographic characteristics of the labor force.

Thus, personnel/human resource management research lagged developments in management practice and public policy in the 1970s. While some research was done on the effects of regulatory policies such as OSHA, ERISA, and EEO, with few exceptions (most notably in the selection area), this work generally did not find its way into the journals most closely identified with the personnel field. In

addition, while there is a good deal of discussion of the growing importance of human resource planning and policy development, especially within large firms, careful studies of the nature, extent, and effects of these planning activities and policies are only now beginning to appear (Foulkes, 1980; Walker, 1980; Dyer, 1982). From all indications then, the 1980s should be an exciting time for personnel researchers if they can shift their attention away from analyses of purely individual behavior and simple relationships among two sets of well worn variables, and out of the laboratory. What is needed is broader gauged research that examines the effects of external developments, organizational strategies and policies, and traditional personnel practices and relates them to important personnel/human resource management outcomes.

A challenge lies in making research on these broader issues theoretically well-grounded and empirically well designed. The danger is that we will return to the prescriptive literature of the 1950s and 1960s and attempt to enumerate various "principles" of personnel/human resource management without a solid theoretical or empirical foundation. In the 1970s important strides were made in strengthening the quality of research on personnel issues. The next step is to build on this base while adopting a more macro perspective.

Labor economists are already moving in this direction. Examples of recent or current work of this nature include studies of the effects of seniority and ability on earnings and promotion probabilities within the firm (Medoff & Abraham, 1981); studies of the effects of affirmative compliance activities on turnover rates (Osterman, 1982), and; studies of "internal labor markets" of white collar and other professional occupations.

New legal developments should also be attracting attention of personnel researchers. Until recently, the American legal system has held that employees could be terminated "at will". Except for certain proscribed motivations, such as racial discrimination, employers were free to modify the terms of employment or sever the employee-employer relationship in the absence of a union contract. Nonunion employees, in short, had no remedies of due process if they felt they were mistreated. During the late 1970s and early 1980s, some courts began to find requirements of due process for nonunion workers implicit in various public policies or in the statements made by employers in personnel handbooks. There have also been periodic suggestions that European-style due process requirements should be imposed by law on employers. Such changes, whether made by statute or court interpretation, could have a profound impact on the nonunion sector and on the conduct of personnel management.

In short, the 1980s will see a challenge to the individual level, psychology-based foundation of personnel research by those addressing similar questions from an economic theory or organizational strategy base. If traditional personnel researchers do not take account of this other work, George Strauss's prediction, while a bit off in timing, may prove to be correct after all.

What is more likely to develop, however, is more diversity in the research and types of researchers working on personnel problems. Some will be psychologically based and will continue to focus on individual outcomes. Some will be labor economists who will look across firms using more aggregate data or existing demographic and other personnel data sets contained within the files of large firms. Still others will take a managerial strategy and policy point of view

and will focus on personnel/human resource management planning and on outcomes that mix individual, subunit, and organizational data. The result could be a decade of exciting debate and ferment within the broad field of personnel/human resource management.

Organizational Behavior

The 1970s was a decade in which many people called for a closer integration between labor/management relations and organizational behavior. Indeed, in the middle of the decade, the IRRA devoted an entire volume to reviewing the viability of such a linkage and the progress made up to that time (Strauss, Miles, Snow, and Tannenbaum, 1974). Driving this work was the view that behavioral theories and methods could broaden the perspectives of industrial relations researchers and strengthen their empirical research methods. While behavioral theories and research methods had a long history of application in the personnel field, at the beginning of the 1970s it was still a rather unique (many at the time would have said misguided) piece of research that attempted to apply these theories and methods to research on collective bargaining and union management relations. Brett and Hammer demonstrate in their chapter that this is no longer the case. We now have available a wide range of studies that cross the boundary between traditional industrial relations topics and organizational issues using a mixture of methodologies from both fields. Clearly, the call for researchers to span the boundary between organizational behavior and labor/management relations was answered during the 1970s.

The decade began with the expectation that the open systems theories of organization developed in the 1960s which stressed concepts such as the environment, technology, structure,

decision-making, conflict, and innovation would eventually be linked in a more clear cut way to micro studies of group behavior, motivation, supervision, organizational change and development, etc. This, however, did not occur. Neither did the enthusiasm for quantitative analysis of organizations (also a product of the 1960s) continue to grow. By the mid 1970s a new school of qualitative analysis led by ethnographers was gaining popularity in organizational behavior.

Instead of building incrementally on the ideas of the 1960s, organizational behavior exploded in a rather scatter shot fashion in the 1970s. It was a decade in which the leading theorists discovered metaphors and, it seemed the more obtruse or arcane the metaphor, the better. Weick, one of the advocates of the use of metaphors, provides a partial summary and illustration of these developments:

Metaphors are abundant in organizational theory; organizations have variously been portrayed as anarchies..., seesaws..., space stations..., marketplaces..., and data processing schedules. (Weick, 1979; 47)

To this list could be added "loosely coupled systems," "garbage can models," "socially constructed realities," "ecological systems subject to natural selection processes," "political systems," "social networks", etc. Each metaphor implied a different view of the nature of organizations and, therefore, a different approach to building and testing organizational theories and deriving prescriptions for organizational actors or external agents interested in controlling the behavior of organizations. The result of all of this was that by the end of the decade the field of organizational behavior seemed to have lost its momentum and its sense of purpose.

Because of these developments, industrial relations researchers must now be more selective and informed consumers of and contributors

to organizational behavior research. It is no longer fair to challenge students interested in both areas to achieve a more complete integration. Instead, researchers are now forced to choose from among the various schools of organizational behavior the one(s) that appears to make the most sense for gaining insight into industrial relations problems and concerns. For their chapter, Brett and Hammer chose macro theories that emphasize the political aspects of organizations and micro models that emphasize the social processing of information, as well as models of worker participation, and decision-making and bargaining. They also stress quantitative research while noting the power of qualitative case and ethnographic studies for generating new hypotheses or developing new theoretical insights.

Brett and Hammer point out that the theoretical integration that has occurred between organizational behavior and industrial relations has been both quite selective and mutually beneficial. Organizational behaviorists who see organizations as political systems have drawn considerably on the views of conflict, bargaining, power, and formal systems of participation developed in industrial relations. In turn, industrial relations researchers have used the political perspective to round out their understanding of collective bargaining and various efforts to supplement bargaining with more direct forms of worker participation.

Unlike the chapters in this volume that are rooted in economic theory, those emphasizing the behavioral tradition focus less on public policy than on deriving implications for decision makers within management and labor organizations. While this partly reflects a difference in levels of analysis, it also reflects

differences in the traditions guiding research in these two areas. Yet there is some evidence that each group is trying to extend its reach to speak to the other's audience. Behavioral studies of conflict resolution and impasse procedures or of formal systems of participation are increasingly addressing issues of public policy as well as private practice. Economic analysis of implicit contracts between employees and employers, organizational responses to government regulations, and the effects of private and public training programs are increasingly reaching into organizations. Perhaps now is the time to examine the links between public policy, economic analysis and organizational behavior research just as the 1970s was the time for linking organizational behavior and collective bargaining research.

Labor History

During the 1970s the industrial relations community was treated to a decade of debate over the appropriate research paradigm for the study of labor history. The 1970s will be remembered as the decade in which the Wisconsin School historians who sought to understand labor history by studying the behavior and evolution of the labor union were challenged, and some would say overtaken by, social historians who attempt to document and interpret the history of workers rather than the history of their unions. Like most intellectual debates, the early entries into this fray emphasized differences in political views, theory and method, and disciplinary underpinnings. The social historians generally reflect a more radical point of view, are rooted in social rather than economic history, and are more strongly identified with history rather than economics or industrial relations. Later in the decade

representatives of the old and new schools joined the debate at professional meetings and in journal reviews. By the end of the decade the accomplishments of the new school were being reappraised and the potential for some type of synthesis was being explored. Interestingly, the synthesis appears to be occurring around the workplace as the meeting ground between the social environment of workers and management and union institutions and policies.

Grossman and Moya set high expectations against which the accomplishments of the social historians are to be judged. They conclude that these expectations have not yet been met and to meet them in the future will require both greater synthesis of the two approaches and some new directions as well. They point out that the next decade will require a number of choices as labor historians seek to determine not only the appropriate balance between the old and the new labor history but also to address issues that have been given inadequate attention by both schools. In the balancing category, for example, historians will need to decide whether to bring economic history back into the field. Grossman and Moya argue that there is an inconsistency between the new school's intention to conceptualize the workers in their complete environment (not just the institutional environment) and the crowding out of detailed consideration of economic issues. If economic history makes a comeback, historians will also need to determine whether they wish to continue to draw on traditional research methods or embrace the cliometrics methodology that has gained popularity in other areas of economic history.

The authors offer two subject areas as examples of topics that have fallen between the cracks of the new and the old schools which, they believe, deserve greater attention in the years ahead. They

particularly stress the need for careful historical studies of public policy issues. In a world where so much analysis of labor policy focuses on recent or current experiences, this type of historical work would surely serve as a useful complement. Indeed, a number of recent works are in this vein such as the study of the development and evolution of the National Labor Relations Board (Gross, 1981) and the ongoing work of Grossman (1973) in cataloging the history of policies and leadership at the U.S. Department of Labor. Jacoby's (1982) current work on the history of the law of employment contracts in the U.S. and Great Britain represents an even more direct example of the work that is being called for here.

Other areas that lie between the new and the old school involve the history of the organization of work, the effects of technology at the workplace, and the role of managerial practices as control devices and strategies. This work tends to take on a more radical perspective (Braverman, 1974; Edwards, 1979; Hill, 1981; Sabel, 1982) and therefore should find a comfortable niche within the new school of labor history. Yet it is also consistent with the early work of the Wisconsin School since Commons' own approach was to try to understand the interaction of changing markets, technology, and managerial practices and their effects on workers and their unions. Perhaps this work, along with the work on labor management cooperation (Moye, 1980; Jacoby, 1981) will serve to put many of the current developments in industrial relations in a more informed historical perspective.

It might also be noted that while the Wisconsin school focused too narrowly on unions, the new historians tend to focus narrowly on the history of blue-collar production workers. This may have been

acceptable given the nature of the labor force in the nineteenth and early twentieth century, however, if the history of workers after 1950 is to be written, it is clear that scholars will have to come to grips with the experiences, aspirations, and organizational contexts of white-collar, professional, technical, and managerial workers (not to mention college professors). The blue-collar worker is a shrinking minority in the labor force and can no longer be used to characterize the "typical" American worker. To understand the history of contemporary workers the diversity of occupations, status groups, and demographic characteristics of the labor force will need to be taken into account.

During the 1980s, labor historians might usefully consider lessons from the past as insights for the present. For example, the current interest in quality of working life and worker participation in management has earlier precedents. Although the labels used for these concepts were different in the past, the concepts themselves are not new. What can be learned about the economic and social conditions that cause revivals of these ideas? A number of nonunion employers have adopted notions of quality of working life and "Theory Z" as alternatives to unionization. To what extent can these management strategies be compared to the "employee representation plans" and "company unions" of the 1920s and 1930s?

While the social historians can be criticized for failing to produce a synthesis by the early 1980s, this criticism must be tempered by the fact that the field is still emerging. The criticism will be more telling if it is still valid in 1990. If the field is to mature to the point where a better synthesis of its basic ideas is evident, its early contributors will have to demonstrate the same

loyalty and long-standing commitment to their ideas and points of view that was the hallmark of the generation of scholars they now so roundly criticize. Their ability to continue to advance their new paradigm will determine whether they have mounted a lasting challenge to the institutionalists or merely represent a passing fad. Put another way, we have a number of years to wait to see if any of these new scholars can make as clear and lasting an imprint on our thinking about labor history as did the best of their precursors such as Commons, Perlman, and Taft.

Unions and Collective Bargaining

The 1970s are described by Roomkin and Juris as the decade in which researchers studying unions and collective bargaining searched for a stronger scientific or analytical base. Model building and quantitative methods were applied to a broad array of the traditional issues in this field identified earlier by the institutionalists. As a result, significant advances were made in the study of such diverse issues as union growth, the propensity of individual workers to join unions, strikes, dispute resolution procedures, and the outcomes and effects of collective bargaining. By the end of the decade, therefore, more precise and empirically grounded statements could be made about such things as why unions grew more rapidly in some periods than in others, how aggregate strike rates vary in relationship to changes in economic conditions, and whether or how mediation, factfinding, conventional arbitration, and final offer arbitration affect strikes, dependence on third parties, and wage and compensation levels.

These empirical advances did not, however, provide answers to the "big picture" questions that have always been central to the

study of unions and collective bargaining. Questions about the future of the American labor movement, the viability and social utility of the U.S. collective bargaining system, or the relevance and effectiveness of the National Labor Relations Act are as open to debate now as they were before the empirical revolutions of the 1970s. It is clear that addressing these big and other "big picture" questions will require a blending of the model building and empirical advances of the 1970s with a more thorough appreciation and use of the historical/institutional methods of the earlier generation.

Indeed, this type of blending may be the next logical development in the evolution of collective bargaining research. The model building/empirical emphasis of the 1970s developed during what might have been the final stage in the incremental evolution and maturation of the post World War II system of collective bargaining. Thus, it was appropriate for researchers to concentrate on explaining across sectional variations in what appeared to be a relatively stable, or incrementally changing, system. By all indications, the 1980s appear to be a time of greater upheaval and potential change. A debate is already shaping up among practitioners and scholars over whether the visible changes in collective bargaining and the less visible changes occurring at the workplace and in the higher level cloisters where union and employer strategies and formulated are only new variations on old patterns or structural adjustments that will transform in some fundamental and lasting ways the roles played by unions and collective bargaining.

Roomkin and Juris suggest a viable way for researchers to address these questions without discarding the advances in research methodology. They argue for more systematic attention to the effects

of time, age, and maturity in collective bargaining relationships. This in turn will require a mixing of quantitative longitudinal research with a well informed historical perspective. The key questions for researchers will be whether the aging of the American collective bargaining system has gone beyond the stage of maturity to one of atrophy or whether the current readjustments will result in a renewal of collective bargaining and the labor movement in the 1980s. In short, the big picture is likely to reemerge as a central topic of interest to collective bargaining researchers, policymakers, and practitioners alike in the years ahead.

Comparisons with Earlier Decades

Now that we have reviewed and commented on the points raised by the various authors, it might be well to consider a number of developments from earlier decades that did not appear to drive research in the 1970s.

There is no mention in any of the chapters of major contributions to a "grand" or "general" theory of industrial relations in the tradition of the work of Commons and his early generation of institutional economists or the more recent contributions of Dunlop (1958) and Kerr, Dunlop, Harbison, and Myers (1960). Similarly, there appeared little of the soul searching over the scope of industrial relations as an academic discipline that occurred in the 1960s after the publication of the Dunlop and Kerr et. al. books (although at the 1976 IRRA meetings there was some soul searching about the Kerr, et. al. work). Indeed, the term "industrial relations theory" does not play an important role in research reviewed in any of the previous chapters. Instead, either the work was atheoretical or its theoretical focus shifted back to

the basic disciplines closest to the problem areas covered.

Similarly, there were no major interdisciplinary research projects launched similar to the earlier contributions of the Illini Cities Study of the 1950s (Derber et. al., 1965), the studies of The Causes of Industrial Peace (Golden and Parker, 1955) sponsored by the National Planning Association, the encyclopedic Brookings Study of The Impact of Collective Bargaining on Management (Slichter, Healy, and Livernash, 1960), or the original father of all mammoth research projects in industrial relations, The Documentary History of American Labor (Commons and Associates, 1936).

No great intellectual debates will stick in the minds of students of the 1970s such as the 1946 Machlup-Lester debate over the utility of marginal analysis, or the Ross-Dunlop debate of the 1940s and 1950s over whether trade union behavior can best be studied from a political or an economic perspective. Neither did the pressures of public events lead to the formation of any highly visible national study and/or research commission that approaches the stature of the Industrial Commissions of the early part of the century, or on the more limited scale, the various state wide public sector labor relations study committees of the 1960s such as the Taylor Committee in New York. The original National Manpower Policy Task Force continued under the sanitized name of the National Council on Employment Policy and other employment policy commissions were active such as the National Commission on Employment Policy, the National Commission on Unemployment Statistics, and the Minimum Wage Study Commission, the Presidents Commission on Pension Policy, and the Select Commission on Immigration and Refugee Policy. None of these, however had much visibility in the research community nor did they

effect public policy. Indeed, the major labor relations law debate of the 1970s--the Labor Law Reform Act of 1977-78--took place devoid of any empirical or theoretical contributions (other than through expert testimony and personal lobbying). And as Barocchi noted, the major shifts in employment and training policy currently under consideration are evolving without significant input from the research community.

Even when we examine where industrial relations research, broadly defined, had its greatest impacts on practice, attention shifts from the work of the third party neutrals trained in the days of the War Labor Board who worked on such things as the Missile Sites Labor-Management Panel, the Armour Automation Commission, or various national emergency boards set up under the Taft-Hartley or the Railway Labor Acts. While industrial relations researchers and practitioners did play prominent roles in administering incomes policies and in the various other tripartite committees that were activated in recent years, the professionals who may have been having the greatest long term effect on practice were those trying to bring about new forms of labor-management relations through the applications of behavioral science theories and techniques. Some of these professionals were organizational development consultants attempting to introduce quality of working life programs in union and non-union firms; others were the management consultants advising firms on how to remain non-union or win representation election contests; still others were helping install and monitor equal employment opportunity and affirmative action compliance and upgrading personnel programs.

In summary, industrial relations research in the 1970s retreated from the national headlines, avoided the search for a grand theory, and instead sought refuge in the basic disciplines and concentrated on making more limited, but better empirically grounded, contributions to middle range theories and public and managerial policies. Perhaps this reflected the decline of collective bargaining as an issue of national concern and visible importance. Where collective bargaining issues were important they tended to be focused around the macro economic and incomes policies discussed in the Flanagan and Mitchell chapter. Perhaps it also reflected the relatively status quo-incremental adjustment pattern that characterized collective bargaining during the 1970s and up through the start of the 1980s (Freedman, 1979; Kochan, 1980, Mitchell, 1981).

Will these trends continue, or are we likely to see major shifts in the patterns of the literature in the 1980s and a return to the broad theoretical issues and national level public policy debates? Clearly, no new paradigm or theory is likely to emerge in a social vacuum. Breakthroughs or major shifts in directions in industrial relations research have always been and will likely continue to be driven by the pressures of public events. If there appears to be renewed interest in the search for a new industrial relations paradigm or grand theory, it will be stimulated by a perception that the current environment and events are challenging the existing industrial relations system in some fundamental ways. If we see renewed national debate over industrial relations issues it will be because the basic legal and institutional framework established in the New Deal legislation of the 1930s and elaborated upon with the regulations enacted since 1964 are now being

reevaluated, modified in fundamental ways, or ignored. If we see the coming together of new versions of interdisciplinary coalitions such as the War Labor Board or the Slichter, Healy, Livernash team it will be because practices have changed so fundamentally that our old descriptions (and therefore the prescriptions that followed them) are no longer accurate or relevant. This is one scenario for research in the 1980s.

On the other hand, we may see a continuation of the trends established in the 1970s: More efforts to improve research methods, more precise but limited theories, and retreat from industrial relations per se back into the basic disciplines. If this occurs, the jurisdictional claim of industrial relations as the central depository of research on employment issues will be further eroded and its teaching and research bases within major universities will be further weakened.

The 1970s may well go down in the history of industrial relations research as the decade in which the baton was passed from that venerable generation of War Labor Board researchers and founders of the Industrial Relations Research Association to a younger group of researchers who have broadened the set of issues that fall into this field, applied different theories and methods, and in some cases challenged the established research paradigms. We leave it to the industrial relations researchers of the 1980s to determine the future course of the field and to the reviewers of the 1992 IRRA research volume to review the record and render the next verdict.

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